

SCIENCE

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THE JULIEN ELECTRIC TRACTION SYSTEM.

THE Julien electric cars have now been in regular passenger service for a little over two years in Brussels; and a report has just been prepared of the cost of motive power during that time, including the renewal of batteries, the wear and tear on motors and machinery, the generating and storing of the energy, and repairs and replacements generally, — in fact, every element that can be understood by an engineer to be motive power. It is found that the cost of motive power has been a trifle less than three cents per kilometer, or about five cents per car-mile; in this, the cost of

three and five-tenths cents per car-mile net, including depreciation on battery, cost of generating current, and handling of batteries.

The car shown in the accompanying illustration, Fig. 3, has been in constant use in this city for several months, and is of the type selected by the Julien Company as the standard for their service. It has a sixteen-foot body mounted on a rigid truck with a six-foot wheel base, which carries two ten horse-power electric motors, — the truck being entirely independent of the car body, and may be removed if necessary. The weight of the car, with motors, gearing and battery in position, is between six and seven tons. The motors are geared direct, one to each axle, and are accessible from

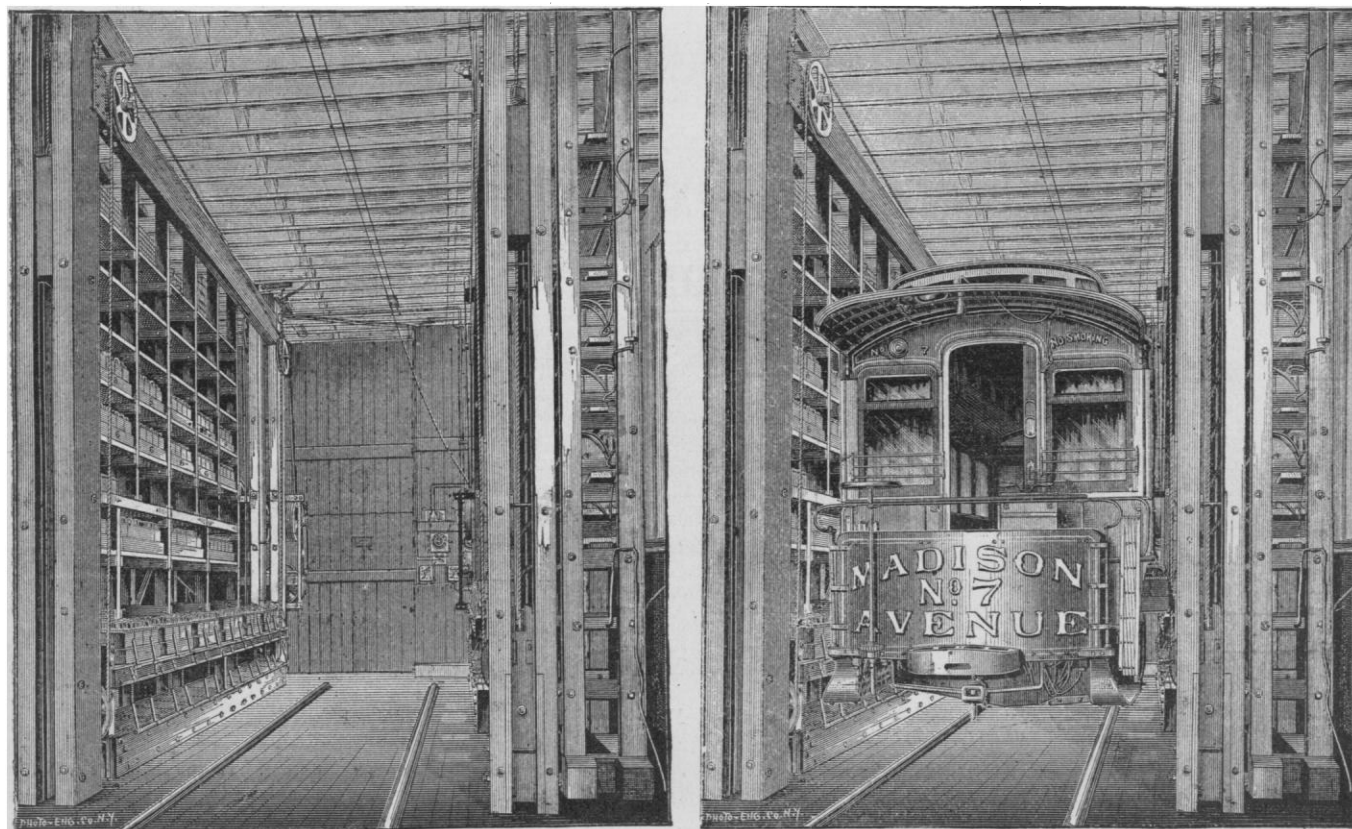


FIG. 1 AND 2.—STORAGE BATTERY CHARGING RACK, JULIEN ELECTRIC CAR STATION.

maintaining the batteries has amounted to a cent and six-tenths per car-mile.

It may be of interest to know that the estimate of the cost of motive power as based on the experiences of the Julien Electric Traction Company on the Fourth and Madison avenues, and prepared prior to the report at Brussels, and without any knowledge of the cost there, is within a fraction of being the same. The Julien Company find the cost of motive power on Madison Avenue to be five and three-tenths cents per car-mile. In the cost of motive power as estimated in New York, however, was included interest on investment, amounting to one and eight-tenths cents, or

the car floor by trap-doors. The battery consists of 108 cells, which are placed in six trays of nine each on each side of the car under the seats; these trays are placed in the car by dropping the side panels.

The chief difficulty encountered in the operation of the Fourth Avenue line was the handling of the batteries, but recent improvements have entirely overcome this difficulty. First, a flexible connector was devised, by which it is possible to couple up cells with great rapidity. Next a battery rack was constructed large enough to store batteries for ten or twelve cars. This rack is shown in Figs. 1 and 2. This rack makes it possible to remove the batteries from

a car and replace them by another set in from two to three minutes. When the car enters this rack, its panels are dropped down on either side and thus form bridges over which the batteries are withdrawn from and replaced in the car. While this change is being made, a competent person inspects the regulators of the car. The motors, gearings, and connections are only inspected once a day, and that at the end of the day's work.

GOLD EXTRACTION BY A NEW PROCESS.

IN many places where gold-bearing quartz is found containing a sufficient percentage of the metal to pay for working it, there is either an entire absence of the water necessary to work the process at present employed for its extraction, or it can only be obtained at great expense and trouble, in many cases only part of the year.

which he could quickly determine whether any specimens of quartz contained gold, by simply crushing it with a hammer and running it through the machine. The mechanism of this apparatus consists of an inclined ladder with fine wire cloth upon one side and silk upon the other. A blast of air is passed up and down through the two meshes, blowing off the light particles of dirt and quartz and allowing the free gold to be retained simply by gravity. Another machine is adapted for concentrating various metals from rock, such as sulphates of copper, lead, zinc, and antimony, making the future separation of the valuable metals from the metallic mass, by roasting or chemical processes, an easy matter. During the exhibit an interesting experiment was made to show the value of the machines, and the thoroughness with which they performed their work. The machine used in the experiment weighed about five hundred-weight, and was so compact that it could be readily

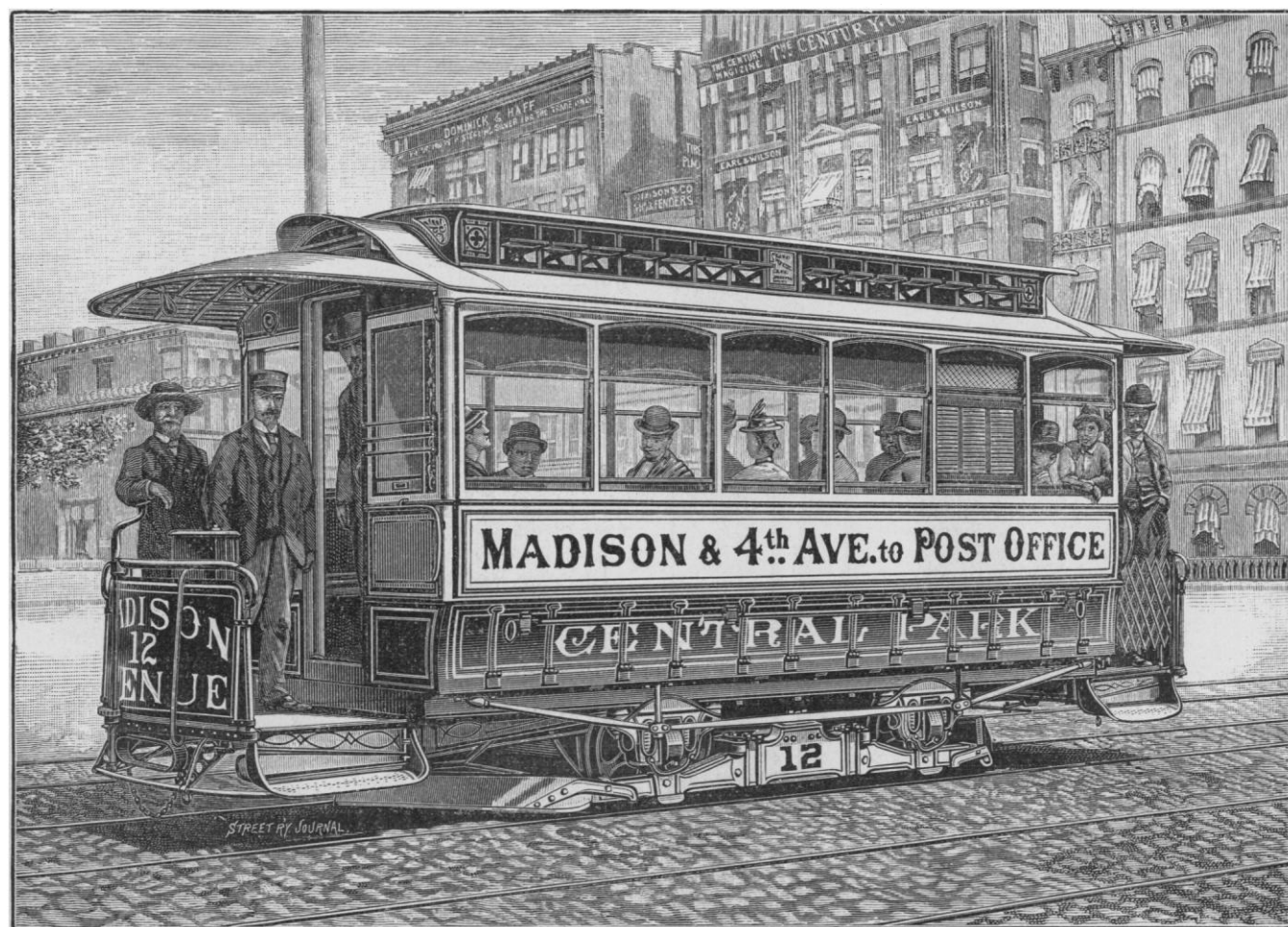


FIG. 3.—STANDARD ELECTRIC CAR, JULIEN STORAGE BATTERY SYSTEM.

Hitherto the processes used for extracting the gold from the alluvial deposit or from crushed quartz have required large quantities of water to flush the fine, pulverized material containing the gold, and even with the best methods large quantities of gold were carried off with the earth and quartz and lost before it reached the mercury.

The need of some ready method for the dry extraction of the gold has long been felt, but until recently the various machines proposed have not been found equal to the old processes. The various difficulties in the way of dry extraction have apparently been overcome in a new machine which was exhibited in London a few weeks ago. By this process, as described in *Iron*, the use of mercury is dispensed with, and the gold is extracted readily from alluvial deposits or quartz. The process is also applicable to the extraction of any combination of metals from refractory ore. One of the machines exhibited weighed but six pounds, and was intended to form part of the outfit of the prospector, by the use of

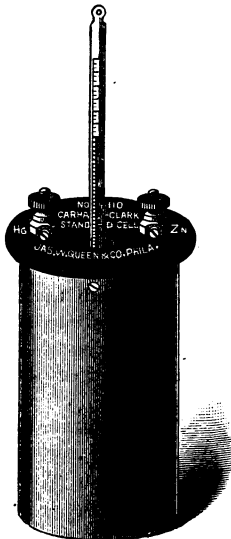
transported from one place to another. A quantity of gold in minute particles, weighing six drams, and two small nuggets, were put into a large pan with two hundred-weight of gravel and grit, and the whole mass put into the machine, which was operated with about a quarter-horse power, or, as an equivalent, two-man power.

The principle of the machine is similar to the small separator used for prospecting purposes, with the blast of air driving off the fine particles of extraneous material, while "oscillating riddles containing shot shake off the heavier grit and stones, allowing free gold to sink by gravity into the shot, where it is retained, and in turn falls to the bottom of the shot." In about a minute after the mixture was placed in the machine the whole treatment was completed, and of the amount of gold originally put into the machine 96.3 per cent was recovered. With more time devoted to the separation, a considerably smaller percentage of loss would doubtless have ensued.

AN IMPROVED STANDARD CLARK CELL WITH LOW
TEMPERATURE COEFFICIENT.¹

LORD RAYLEIGH'S form of Clark cell, described in the Philosophical Transactions for 1885, is the best one hitherto made. The objections to it are, first, that it has a high and variable temperature coefficient; second, it is not constructed in such a way as to keep the mercury away from the zinc when shaken in transportation; and third, an important chemical defect is the local action taking place by which zinc replaces mercury in the mercury salt and the zinc becomes amalgamated, the amalgam often creeping up so as to reach the solder at the copper wire. These difficulties I have, I think, perfectly overcome. I have made cells which have been tested for several months with the low coefficient, at 15° C., of 0.000386 per degree C. At higher temperatures a peculiarity is that this coefficient decreases slightly, while that of Lord Rayleigh's increases very appreciably. The cell is so made that the mercury is confined to the bottom of the cell, or at least, if it does move at all it cannot reach the zinc. These cells have been found to stand transportation exceedingly well.

The same arrangement or device removes the zinc from the mercury salt and perfectly prevents local action. The sealing of the cell is also effected with a more perfect compound. Further,



IMPROVED STANDARD CLARK CELL.

in the preparation of the mercury salt I have succeeded in making mercurous sulphate so free from the mercuric form that it shows no yellowing when washed free from acid. It also remains white upon admixtion with zinc sulphate, and indefinitely, after the cell is set up, provided it be kept out of the light. The light darkens it.

One of these cells has been heated up to 53°C. , and the following day it returned to its precise former value of electromotive force at the same temperature. The temperature coefficient given holds at the above high temperature. As indicating the uniformity attained, the last two cells made never differ in electromotive force by more than one part in ten thousand, and usually by only half this, at the same temperature.

THE WENSTROM DYNAMO.

THE Wenstrom dynamo, of which Fig. 1 is a perspective view and Fig. 2 a cross section, is well known in Europe, especially in Sweden. It was invented by Jonas Wenstrom, an eminent Swedish engineer, and differs in some respects from other dynamos in the market. It is of simple and substantial construction, as may be seen by the illustrations, and utilizes the magnetic forces to a remarkable degree. It is of the iron-clad type, the armature and field coils being protected by a cast iron shell, parts of which per-

¹ Abstract of a paper read before the American Association for the Advancement of Science, at Toronto, by Professor H. S. Carhart.

form the function of pole-pieces. There are four poles, opposite ones being of the same sign, all four being energized by one pair of field-coils, which surround the cores of the inner or horizontal pole-pieces, and are surrounded by the shell which serves as annular cores for the top and bottom field-pieces. Ventilation is provided

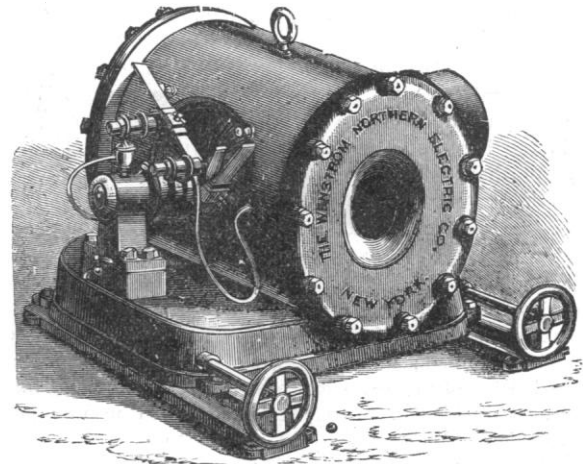


FIG. 1. — WENSTROM DYNAMO.

for by the circular apertures in the shell through which the armature is put into place.

The armature is of the drum pattern, built up in the usual way of thin disks of iron, well insulated, so as to prevent heating from eddy currents. These disks are perforated near the periphery, the perforations being round, ovoid, or hexagonal in shape, and connected with the periphery by a slit, narrowest at the outer part, and only wide enough to admit the winding, one wire at a time. In the grooves formed by these perforations the wire is wound. This peculiar construction admits of the armature revolving in very close proximity to the pole-pieces, materially reducing the resistance of the magnetic circuit, and affording a protection to the armature winding from the effects of centrifugal force, no binding wires being required. A new method of winding is employed, and diametrically opposite sections are connected together, making necessary only two brushes, which are set 90 degrees apart.

The one hundred light machine absorbs eight horse-power, run-

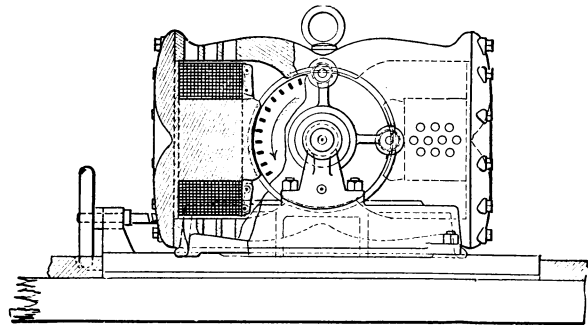


FIG. 2.—WENSTROM DYNAMO.

ning at a speed of nine hundred revolutions per minute. The total weight of the dynamo is eleven hundred pounds, mainly cast-iron, the weight of copper wire on the armature being only thirteen pounds, and on the field magnet cores ninety-four pounds, or one hundred and seven pounds of copper in all. The two hundred and thirty light machine runs at a speed of seven hundred revolutions per minute, its total weight being twenty-five hundred pounds, of which thirty-six pounds are of copper on the armature, and three hundred and eight on the field magnets. The eight hundred light machine runs at a speed of five hundred revolutions per minute.

The advantages claimed for this construction are that there is no waste field, all the magnetic lines of force being utilized in the armature in producing work ; neither is there any field outside of

the machine which would be liable to affect watches, etc., all the field being contained within the outer iron shell forming the yoke. Low speed in running is obtainable without increasing the size and weight of the machine, and the whole is cheap to construct, and combines features of mechanical strength and solidity with high electrical efficiency. Finally, the machine is remarkably free from any heating when running constantly and under full load. These machines are manufactured by the Wenstrom Northern Electric Company, of this city, of which Dr. J. B. De Lery is president; B. Blum, general manager, and B. J. Sfurges, secretary and treasurer. This company intend to introduce their system for light and power in the Eastern, Middle, and Western States. The Wenstrom people have already installed during the past year several thousand lights in Baltimore and Annapolis.

THE NORTH AMERICAN MESOZOIC.¹

It has become customary upon such occasions as this for the speaker to select a theme from subjects which he is supposed to have specially studied; and I have therefore chosen for mine the mesozoic division of the geological record as it is exhibited on this continent. This theme is so comprehensive that I propose only to select from it certain topics which pertain to the distinguishing characteristics of the principal subdivisions of the mesozoic that have been recognized in different portions of North America; to their interdelimitation and to the delimitation of the division as a whole from the carboniferous system beneath, and the cenozoic above. I shall also make the discussion of these topics the opportunity of expressing certain views which I hold concerning them.

To bring these discussions within the time allotted me they must be confined to three general sections of the mesozoic formations, one of which occurs within each of three regions of the continent, namely, the Atlantic coast, the Pacific coast, and the interior regions. Proceeding upon this plan, let us first consider the general section which is to be observed in the Atlantic coast region.

The rocks which in this region are now generally regarded as of triassic age are found occupying limited isolated districts from Prince Edward Island on the north to the State of South Carolina on the south. If they extend further to the south, or south-westward, they are covered from view by later formations. They are found to rest unconformably upon various formations from the archæan to the carboniferous inclusive; except perhaps in Prince Edward Island, where they are reported as resting conformably, or nearly so, upon reputed Permian strata. Still, no intimate stratigraphical or paleontological connection between the Permian and the trias has been shown to exist there; and the hiatus between them is doubtless as great as it is farther southward, where the unconformity is so conspicuous.

In this latter portion of the region it is evident that the great uplift which involved the paleozoic rocks, including the reputed Permian, took place long before the deposition of the earliest of those triassic beds. These stratigraphical conditions indicate that the hiatus in the geological record between the latest of the carboniferous, and the earliest of the triassic deposits is equal to at least the earlier half of the triassic, as that period is represented in Europe.

The only known paleontological evidence which appears to bear upon this subject agrees with the stratigraphical indications just mentioned. That is, the results of investigations by Professor Newberry upon the fishes and plants of the strata in question, and of Professor Fontaine upon the plants of the same, indicate that they represent the later trias of Europe. But if triassic fishes had not survived to the present day; and if we knew more concerning the developmental stages in the vegetable kingdom from the later paleozoic to the later mesozoic inclusive, a good degree of uncertainty which is naturally felt upon this point would doubtless disappear.

Our knowledge of the land vertebrate fauna which existed at the time these deposits were formed is derived mainly from footprints; and it is therefore more than usually imperfect. The character of

this evidence as indicating triassic, rather than earlier Jurassic age, seems to be far from unquestionable.

Very few invertebrate fossils have been found in the trias of the Atlantic coast region; and the few that have been discovered are of little or no value as indicating the age of the strata containing them.

As to the relation of these deposits with the carboniferous system, only stratigraphical evidence has thus far been obtained, and this shows only the bare fact that the former are of considerably later age than the latter. That is, no direct, or even approximately close, biological relationship between them has yet been discovered, the biological hiatus being apparently quite as great as the stratigraphical one. It may be mentioned here also that we have no evidence that the trias of the Atlantic coast was ever continuous, or that it was exactly contemporaneous, with the reputed trias of the interior region, which will be presently referred to.

Intermediate between the triassic beds and the undisputed cretaceous deposits of the Atlantic coast region there is a series of strata, evidently of littoral and estuary origin, but, at least in part, of doubtful age, to which the name of Potomac formation has been applied. These deposits reach at most only a few hundred feet in thickness, and although frequently covered from sight by later formations, they seem to have been originally continuous from New Jersey to the State of Mississippi. They have no known representative west of the Mississippi River, unless it shall be shown that they are represented by some sandy beds at the base of the Texas cretaceous section. These Potomac beds are usually found resting upon the archæan, and at only a few points are they found to rest directly upon the triassic rocks, when they are plainly unconformable. They seem to be constantly present beneath the marine cretaceous strata just mentioned, and no representative of another formation has yet been observed between them.

Invertebrate fossils are exceedingly rare in the Potomac formation, and the few that have been found give no direct indication of its geological age. Professor Whitfield, however, has suggested that the Raritan clays, together with the Amboy clays, which by some geologists are included in the Potomac formation, but which are probably of later date, are of Jurassic age because of the similarity of his new lamellibranchiate genus *Ambonicardia* with certain European Jurassic shells.

Large collections of fossil plants have been obtained from the deposits here provisionally grouped together under the name of Potomac formation, at numerous and widely separated localities. These collections differ so greatly in character from one another that it seems necessary to infer that more than one flora is represented by them. Many years ago Dr. Tyson found some fossil plants in Maryland which he regarded as of Jurassic age, and which closely resemble certain forms that are found in the European Jura. Professor Ward, in reviewing the large flora which Professor Fontaine has published from the Potomac formation in Virginia, and having in mind also the Maryland plants just referred to, recognizes the Jurassic character of several of the species, according to the European standard, but he takes the rational ground that all obtainable evidence ought to be considered before reaching a final decision as to the true age of the deposits containing them.

Professor Newberry, who has made extensive studies of the plant remains of the Raritan and Amboy clays, finds among them none that give any indication of their Jurassic age. On the contrary, he finds that the flora of those clays as a whole indicates that they ought to be referred to an epoch not later than the middle cretaceous of Europe, nor probably earlier than the upper neocomian.

Professor Marsh has published some dinosaurian remains from apparently the same horizon in the Potomac formation that furnished the plants to Dr. Tyson and Professor Fontaine, which he has referred to the Jurassic.

Paleontological testimony being thus conflicting in its character, one naturally infers that more than one epoch is represented by the deposits that now bear the common name of Potomac formation; but I shall presently call your attention to some cases of commingling of earlier and later molluscan types in one and the same formation which are quite as remarkable as this apparent commingling of diverse plant and vertebrate types in the Potomac formation.

¹ Address before the Section of Geology and Geography of the American Association for the Advancement of Science, at Toronto, Ont., Aug. 29, 1889, by Charles A. White, vice-president of the section.

The marine upper cretaceous deposits of the Atlantic coast region which immediately overlies the Potomac formation are best developed in New Jersey; but there is good reason to believe that they were originally continuous with contemporaneous deposits through the whole length of the region from Long Island to the Gulf States and thence westward to, and far northward within, the interior region. This opinion is based upon specific identity of marine fossils discovered in the different regions.

The upper cretaceous of this region is overlain by eocene deposits, also marine, with little if any observable unconformity where they have been found in contact. I shall, however, presently mention facts which indicate that there is in the Atlantic and Gulf coast region a considerable hiatus between the cretaceous and eocene.

Briefly, then, the mesozoic of the Atlantic coast region consists of a probable representation of the upper trias of Europe, a possible one of the upper Jura, a probable slight one of the middle cretaceous, and a practically certain representation of a large part of the upper cretaceous, but with an hiatus between the latter and the eocene.

Although the cretaceous rocks are, or were originally, continuous between the Atlantic coast and interior regions by way of the Gulf States, the earlier mesozoic rocks of those regions respectively are so widely separated from each other that, as we go westward, we do not find any that can be confidently referred to either the trias or the Jura until we have passed the 100th or perhaps the 103d meridian.

As the latter meridian coincides with the western boundary of Texas, the foregoing statement implies that no triassic rocks exist within at least the greater part of the fully thirty thousand square miles in that State and in the Indian Territory, which some geologists have represented as being occupied only by rocks of that age. A personal examination of a large part of that region and of the fossils collected there has satisfied me that the sum of all the known evidence is in favor of the Permian age of the strata in question and against their triassic age. But these strata have an important paleontological relation with the mesozoic, to which I wish to call your attention for a few moments.

Upwards of fifty species of vertebrates, embracing reptiles, batrachians, and fishes, have been described from these rocks by Professor Cope, upon the evidence of which he referred them to the Permian of Europe, although, as he states, not one of the genera is common to both continents.

I have collected upward of thirty species of invertebrates from the same beds which furnished the vertebrates, representative examples of all the more important of which were obtained from one and the same stratum. Of these, fully one-half are common, characteristic coal-measure species. A part of the cephalopod species, however, possess such decided mesozoic characteristics that probably no special student of that class of fossil mollusca would hesitate to refer them to a formation not older than the trias, if they had been submitted to him without any information as to their true stratigraphical position.

It is a significant fact that if three special selections were made from the fossils of all kinds that have been obtained from this formation in Texas, one could be made, by the usual method of chronological classification practised by paleontologists, to prove its coal-measure age, another its Permian age, and still another its triassic age. It is admitted that the sagacity of an experienced paleontologist will often enable him upon limited evidence to become satisfied in his own mind as to the approximate age of a given formation; but it is only after all the obtainable paleontological and stratigraphical facts are carefully considered together that one is justified in expressing a definite opinion upon a subject of this kind. Such a summing up of all the evidence at present available seems to fully justify the reference of this Texan formation to the Permian of Europe.

My special object in presenting the foregoing facts is to call your attention to the important paleontological relation of the Texan Permian with the mesozoic, which is shown by the presence of ammonitic and ceratitic cephalopods among paleozoic types of mollusks. The discovery of such forms in such association in the Texan Permian, as well as in the *Productus* limestone of India,

shows conclusively that certain mesozoic types began their existence long before the close of paleozoic time. Such forms in such association may be properly regarded as harbingers of an approaching, but not yet established, mesozoic era, because, in this case at least, the balance of paleontological evidence favors their reference to the paleozoic. Such facts as those which have been mentioned, as well as others presently to be referred to, indicate that upon the confines of epochs, periods, and ages of geological time there was always a commingling of types of then living forms which in their culmination were characteristic of each of those chronological divisions respectively. Furthermore, I shall call your attention to evidence that some of the types which especially characterized certain geological periods survived in full vigor through later periods. But let us return to a consideration of the mesozoic rocks.

Those rocks of the great interior region which have by common consent, but upon comparatively slight evidence, been referred to the trias, are found upturned against the flanks of the Rocky Mountain, and other ranges, and exposed to view in the valleys and cañons of the plateau province. They reach several thousand feet in thickness, and are so nearly uniform in color and lithological character over the whole of the great area within which they occur that they are often designated as the "red beds." They are found resting upon rocks of different age in different places, but in some districts they rest with apparent conformity upon a series of sandstone strata which are probably of Permian age.

This formation is apparently of non-marine origin, and, as a rule, it is quite barren of fossils. The few molluscan remains that have been obtained from it give no indication as to its age, and, in the light of present knowledge, the few plant and vertebrate remains obtained from it are far from satisfactory in this respect. Still, it is not my object to deny the triassic age of this formation, but only to call your attention to the fact that paleontological evidence upon this point is very meagre.

Because of the paucity of fossils both in this formation and in the reputed Permian upon which it rests in different districts, little is known of any paleontological relationship between them. There are, however, some indications of such relationship that deserve mention. The case of the commingling of mesozoic and paleozoic types in the Permian of Texas has already been stated. Another case in South Park, Colorado, may be mentioned, and the possible occurrence of still another in south-eastern Idaho may be suggested.

Important collections of plants and insect remains have been obtained from certain strata in South Park which are reported as immediately overlying rocks of unquestionable carboniferous age. The plants are regarded by Professor Ward as constituting the most characteristic Permian flora that has been found on this continent. The stratigraphical relation of these rocks is also suggestive of their Permian age; and yet Mr. Scudder referred the insects to the trias without qualification.

Some years ago Dr. Peale discovered in south-eastern Idaho an unique assemblage of fossils in strata which rest conformably upon the carboniferous, and evidently occupy a position beneath the triassic red beds, which occur in the same neighborhood. A part of the species belong to the *Ammonitidae* and a part to the *Ceratitidae*; and upon the evidence of these cephalopods Professor Hyatt referred the strata bearing them to the middle trias of Europe. When one remembers that cephalopod forms similar to those just referred to occur in India associated with a characteristic carboniferous fauna, he naturally inquires whether it is not possible that the Idaho strata ought to be referred to a period not later than the Permian.

Those Idaho strata and the South Park and Texan Permian all possess great interest as indicating an intimate relationship between the mesozoic and the carboniferous of the interior region; and if the record between the paleozoic and the mesozoic had not been so generally and so badly broken on this continent, we should doubtless now find many similar and more complete cases of the commingling of earlier and later types.

Some American field geologists have privately, if not publicly, expressed the opinion that the Permian ought to be assigned to the mesozoic, rather than to the paleozoic; but notwithstanding the paleontological relationship that has just been mentioned, such

a view is untenable when all the known facts are considered. It is at present sufficient to say that the great break between the mesozoic and paleozoic of North America occurred while yet paleozoic forms of life were far in excess of mesozoic forms; and that almost all the North American strata that have been recognized as of Permian age appear to have been the result of continuous sedimentation from the carboniferous. In short, all the hitherto recognized or reputed Permian of North America is far more intimately related, both paleontologically and stratigraphically, with the paleozoic than with the mesozoic. Therefore the lower delimitation of the North American mesozoic must coincide with the base of the lowermost discovered triassic strata.

A few hundred feet in thickness of strata, which have by common consent long been referred to the Jurassic, are found within a large part of the middle portion of the interior region, resting conformably upon the triassic strata which have already been noticed. Where these Jurassic strata have been fully studied, especially in Colorado and Wyoming, they are separable into an upper and a lower portion, the lower portion being of marine, and the upper of fresh-water origin. The invertebrate fossils of the upper portion are mostly of types that are now living, and are, therefore, of no value as indicating their geological age. Those of the lower portion are few, and the cephalopods only, or mainly, present such characters as to suggest their Jurassic age; and it was upon this slight evidence, together with the relative position of the strata, that their reference to the Jurassic was first made.

Professor Marsh's well-known publications of the remarkable dinosaurian faunas from both the upper and lower portions of the strata in question have left no reasonable doubt that they are really of Jurassic age. Professor Marsh refers all these strata to the upper Jurassic of Europe; and in connection with this statement I wish to call your attention to the fact that wherever they have been found in contact with the triassic strata already discussed, they are not only strictly conformable, but they seem to have been the result of continuous sedimentation. In fact, it is paleontology alone that suggests an hiatus between them. The field geologist finds no evidence of it.

The Jurassic rocks of the interior region disappear both to the northward and southward, their geographical range being apparently a little less than that of the underlying triassic beds. No equivalent of the former has been found in Canada, although the cretaceous Dakota group, which immediately overlies the Jurassic further southward, has been recognized there. It may be remarked also that where the Jurassic is not present beneath the cretaceous, the latter, especially in the eastern part of the region, is often found resting directly upon the older rocks, sometimes even upon the archæan. In Texas the Jurassic is also absent from beneath the marine formation, which is regarded as the representative of the Dakota group there, the latter resting directly but unconformably upon the Comanche beds, to be presently noticed.

Omitting present consideration of the isolated masses of reputed Jurassic rocks in western Nevada and eastern California, this subdivision of the mesozoic seems to be represented in North America mainly by the slight accumulation of strata in the interior region which has just been noticed. We know little or nothing of the flora which existed when these strata were deposited; their invertebrate fossils are of little value in determining their geological age, and if it were not for their dinosaurian faunas their Jurassic age might well be questioned.

The section of the cretaceous formations which prevail in the central portion of the interior region, and to which I shall more particularly refer in following remarks, differs materially from a similar section in the southern portion, usually known as the Texas section. Meek and Hayden divided the cretaceous of the central portion of the region into the Dakota, Benton, Niobrara, Pierre, and Fox Hills groups, the first mentioned being the earliest, and the last one mentioned the latest. In Texas the cretaceous section is continued much beneath the equivalent of the Dakota group there. These lower Texan strata constitute the important marine formation now known as the Comanche beds, the molluscan fauna of which gives peculiar paleontological character to the Texas section. Above the Comanche beds there is a series of formations

that are understood to respectively represent all the more northern formations which have just been mentioned.

After due consideration of all the known facts, some of which are of recent acquisition, there seems to be no room for reasonable doubt that the marine cretaceous deposits of the interior region which are later than the Dakota group are, as a whole, not only equivalent with the marine cretaceous deposits of the Atlantic and Gulf coast region, but that they were all originally continuous through the whole of that great geographical extent. These formations are too well known to need present characterization; and they are now known to constitute the most extensive and definite taxonomic horizon that has been recognized among the mesozoic formations of this continent. Furthermore, the marine molluscan fauna of these strata is of such a character as to leave little room for doubt that they represent homotaxially the Senonian, and perhaps a part of the Danian, of Europe. The difficulty, however, of accurately correlating the cretaceous formations of this continent with those of Europe is very great, as has, for example, lately been indicated by Professor Rømer's reference of certain fossils of the Comanche beds to the upper Turonian. These beds lie wholly and unconformably beneath the horizon of the Dakota group, which is itself not probably newer than the Cenomanian.

Before proceeding to a consideration of the Laramie group, it is proper to say that the presence in British America of the Kootanie formation beneath the Dakota group, and that of the Comanche beds beneath the equivalent of the latter in Texas, shows that there is really an hiatus between the Dakota and the Jurassic in the interior region, although their conformity is so complete that it has never been detected by field observation. If a similar hiatus exists between the Jurassic and triassic in the same region, we have also no stratigraphical evidence of it.

The Laramie is in many respects one of the most remarkable of the North American formations. It is found occupying large portions of the interior region from the State of Nuevo Leon in Mexico to beyond 52° north latitude. It reaches a maximum thickness of nearly four thousand feet in Colorado, and more than that in British America. It is not only everywhere conformable upon the Fox Hills group, but wherever the junction between them has been seen, sedimentation from the older to the later formation appears to have been continuous.

In all its great geographical extent the Laramie group has never been found to contain any animal remains similar to those which inhabit the open sea only. A considerable proportion of its invertebrates are like those which are now denizens of brackish waters, and a still greater proportion are fresh-water forms. It is mainly upon this abrupt change from a marine to a brackish and fresh water character of the molluscan fossils, and not upon stratigraphical difference, that we rely to determine the lower limit of the Laramie formation.

The labors of Dr. G. M. Dawson and Mr. Whiteaves, and their associates in the Canadian Survey, have shown that conditions similar to those which gave character to the Laramie formation existed in a large part of the northern interior region long before the close of the Fox Hills epoch, and that they were probably continued into the Laramie epoch. But time will not permit me now to discuss this interesting question.

Besides the invertebrate fauna which has just been referred to, a few insect remains, a rich flora and a somewhat extensive and varied vertebrate fauna have been obtained from the Laramie formation. None of the molluscan remains, so far as I can judge, possess characters which any similar forms might not have possessed at any time from the middle cretaceous to the eocene inclusive; and a large part of them differ from living forms only as species.

Similar remarks may be properly made concerning the plant remains of the Laramie formation. Professor Ward has shown that of the one hundred and twelve genera of plants which have been discovered in the Laramie, thirty-eight of the genera and five of the species are common to the Dakota Group; eighty-five of the genera are living and twenty-seven are extinct. These extinct genera are all so nearly allied to living genera respectively that it is difficult to separate them. Furthermore, not less than three species from the upper strata of the Laramie have been identified with living species.

Mr. Scudder has referred the insect remains to the tertiary, but the vertebrate remains, especially those of mammals and land reptiles, are of more ancient types than those of the plants and invertebrates. Among the few Laramie mammals that have been discovered there is no indication as to the ancestry of that great mammalian fauna which characterized the immediately following Wasatch period. The reptiles are mainly dinosaurs of cretaceous types, but some of them seem to possess characters that suggest their Jurassic age.

Some paleontologists have long hesitated to give an opinion as to the true taxonomic position of the Laramie formation; but those who have studied the vertebrates only have usually referred it unqualifiedly to the cretaceous, apparently assuming that, containing dinosaurian remains, it could not be of later age. Field geologists, especially those who practically ignore paleontological evidence, also refer the Laramie to the cretaceous, because of its intimate stratigraphical relation to the marine cretaceous beneath it, and because in all the principal displacements, which the latter has suffered in the interior region, the Laramie was equally involved.

The formations which overlie the Laramie were, by common consent, long regarded as of tertiary age; but concerning the age of some of them, difference of opinion have since arisen. Between the Laramie and any overlying formation there is often, but not always, unconformity. In Utah, and apparently in the valley of the lower Yellowstone also, I have found the Laramie passing gradually up into purely fresh-water deposits without any stratigraphical break. In the former case I am sure, and in the latter case I believe with Professor Newberry, that the upper strata represent the lower part of the Wasatch group.

In Utah several of the fresh-water molluscan species, which are widely distributed in the Laramie, are found to have passed up into the Wasatch, thus confirming the stratigraphical evidence of the immediate succession of the Wasatch upon the Laramie. In southern Wyoming dinosaurian remains are found in some of the uppermost strata of the Laramie; and the lowermost Wasatch strata in the same region bear coryphodont and other placental mammalian remains; but remains of these two orders have never been found commingled. Still, in view of the facts just stated, it is not possible to doubt that those placental mammals lived contemporaneously with at least the last of the Laramie dinosaurs.

In north-western New Mexico and south-western Colorado, Professor Cope has found certain strata at the base of the Wasatch, and overlying the Laramie, to contain the remains of a peculiar vertebrate fauna whose distinguishing members are placental mammals which are quite different from those of the Wasatch. These strata he designates as the Puerco group, and he now refers them, together with the Laramie, to the cretaceous, because of certain characteristics which the Puerco mammalian and reptilian remains present; but he formerly regarded that group of strata as of Cenozoic age. These Puerco strata have the appearance of having been deposited simultaneously with those which elsewhere constitute the lower portion of the Wasatch group; and before their vertebrates were studied by Professor Cope their identity with the Wasatch was not questioned.

But we are not yet done with dinosaurs. Mr. George H. Eldridge has lately shown that in the vicinity of Denver, Col., there is a distinct formation, from 600 to 1200 feet in thickness resting unconformably upon the Laramie, which he has called the Arapahoe formation. Mr. Whitman Cross has also lately shown that still another formation in the same district, having a maximum thickness of fourteen hundred feet, rests unconformably upon both the Arapahoe and Laramie formations. To these strata he has given the name of Denver formation. The great aggregate thickness of these formations, together with their respective displacement with relation to the Laramie and to each other, shows that much time must have elapsed between the deposition of the uppermost Laramie strata in that district and the uppermost Denver strata.

Mr. Cross shows that a large part of the plant remains, which have been reported as coming from the Laramie in this district, really came from the Denver formation. Some of the fresh-water mollusca of the Denver strata I am not able to distinguish from Laramie species. But the most unexpected fact of all which these gentlemen have brought out is that both these formations above

the Laramie contain dinosaurian remains in comparative abundance. The skull in some species is found to bear a pair of horns similar in posture and shape to those of the hollow-horned ruminants. Some of the bones also present characters which are suggestive of earlier mesozoic age; but in a general way, at least, these dinosaurs are similar to those of the Laramie.

The Laramie group does not reach its maximum thickness in the Denver district, and it is not known whether the latest Laramie strata are represented there. Both the Denver and Arapahoe formations are of limited extent, and it is quite probable that the latter, and perhaps the former, together represent the later portion of the Laramie period. But it is reasonable to infer that at least the later portion of the Denver formation was contemporaneous with the earlier fresh-water eocene strata of the Green River basin, notwithstanding the fact that the former bears dinosaurian remains.

The present state of our knowledge seems to justify us in regarding the marine cretaceous formations immediately beneath the Laramie as representing the Senonian of Europe, perhaps including even a part of the Danian. Now if we add to the American cretaceous the Laramie, Arapahoe, and Denver formations, we evidently extend the cretaceous in America much beyond its recognized latest limit in Europe.

But why, we may ask, should not those dinosaurs have survived from mesozoic, into tertiary time? Why should they not have continued their existence as long as physical conditions were favorable, and as long as they could compete in the struggle for existence with such mammalian faunas as that whose earliest known history is recorded in the earlier strata of the Wasatch formation?

Before summarizing the conditions of the mesozoic of the interior region and proceeding to a consideration of the Pacific coast section, I wish to refer to the relation of the Laramie group with the marine tertiary of the Gulf and the Atlantic coasts.

For reasons presently to be mentioned, no direct stratigraphical proof of contemporaneity of our great fresh-water inland deposits with marine coast deposits is possible, and direct paleontological proof is not to be expected. I had long hoped, however, that because the Laramie group was in part of brackish water origin its continuity or contact with some marine coast deposit might be discovered. Such a discovery was first announced by Professor Cope, which I afterward confirmed, and showed that in the vicinity of Laredo, Texas, the Laramie group as a whole underlies with apparent conformity marine strata which contain an abundance of *Cardita planicosta* and other characteristic eocene fossils; but I was not able to detect the continuity of the Laramie with any sea-coast formation.

It was this discovered relation of the Laramie to the Gulf coast eocene that was referred to by the suggestion in a previous paragraph that there is really an important hiatus, although apparent conformity, between the cretaceous and the tertiary deposits of the Atlantic coast. The Gulf coast eocene just mentioned being regarded as equivalent with that of the Atlantic coast, and the uppermost marine cretaceous immediately beneath the Laramie, as equivalent with the uppermost marine cretaceous of the Atlantic coast, it follows that the hiatus referred to equals the whole of the Laramie. It may also be mentioned in passing, that, both upon stratigraphical and paleontological evidence, I regard both the northern lignitic of Hilgard in Mississippi and its equivalent in eastern Texas as equivalent with the upper, lignite-bearing, portion of the Laramie as it occurs in the valley of the Rio Grande.

Very briefly summarizing the mesozoic of the interior region, we find that its lower delimitation is greatly lacking in uniformity, the lowest member being sometimes the triassic, sometimes, but rarely, the Jurassic, and sometimes the cretaceous. The triassic apparently represents the upper trias of Europe, the Jurassic, the upper Jura, and most of the cretaceous, the upper part of that subdivision of the mesozoic. Above the marine cretaceous strata, inland sea and lacustrine deposits were continued into tertiary time, apparently without a break, either paleontological or stratigraphical.

Having to deal with extensive inland deposits alone when investigating the immediate relation of the mesozoic to the cenozoic in the interior region, we find that the most direct means of determining such relationship is wanting, because the continuity of the

marine paleontological record is broken at the base of the Laramie formation. Still, the opinion that we have a continuous record there from cretaceous into tertiary time is strongly supported by paleontological and stratigraphical evidence. But we come now to consider the mesozoic of the Pacific coast region, where we shall find proof of unbroken continuity of marine deposits from the upper cretaceous to the tertiary. Time will not permit me now to discuss the mesozoic of western British America, which Dr. G. M. Dawson, Mr. Whiteaves, and other Canadian geologists have done such excellent work upon, and I must therefore confine myself mainly to the California section.

The rocks of this portion of the Pacific coast region have been so greatly displaced since their deposition that their study is more difficult than that of the rocks of the interior region. Still, our knowledge of the upper part of the Pacific coast mesozoic is quite satisfactory. The oldest mesozoic strata of the California section which I shall specially refer to on this occasion were, by the California geologists, assigned to the lower cretaceous, under the name of the Shasta group. But these strata do not probably represent the very earliest part of the cretaceous period.

The exact relation of the Shasta group to the cretaceous formations above it has not yet been made clear; but Mr. Diller's investigations in northern California seem to indicate that the hiatus between them is not so marked as has been supposed. The geologists of the California Survey did not recognize any formation as belonging between the Shasta and Chico groups, but Dr. G. F. Becker has reported upon a series of strata in Mendocino county which he believes to be later than the Shasta, and earlier than the Chico. Upon examining the fossils which he collected from those strata, some of the species of which have also been found at Todos Santos Bay in Lower California, I concurred in his opinion, and suggested for those strata and their equivalents the name of Walala group. Still, actual contact of this group with any other cretaceous strata has not yet been discovered, and its actual taxonomic position is not known.

From the base of the Chico group upward, the series of California strata which has been referred to the cretaceous is so well known that little if any difference of opinion exists as to essential facts concerning it, although a wide difference of opinion has arisen as to their significance and importance. This series, aggregating more than ten thousand feet in thickness, was divided into two groups by the California geologists; namely, the Chico below and the Téjon above, although they recognized the fact that there is no distinct break, either paleontological or stratigraphical, between them.

A considerable number of fossil invertebrates, among which are a species of baculites and several ammonitic forms, constitute such a decided mesozoic feature of the fauna of the lower portion of this Chico-Téjon series that the California geologists naturally and properly referred it to the cretaceous. The upper, or Téjon, portion contains a fauna that is so obviously cenozoic in character that several geologists, especially Heilprin and Conrad, have strenuously contended that it is of eocene age. A large proportion of these Téjon species are found to be so common in the Chico portion that if they were not there commingled with the cretaceous forms just referred to, the tertiary age of those lower strata would hardly be questioned. In short, there is in this stratigraphically unbroken Chico-Téjon series of California, a gradual transition of faunal characteristics from the cretaceous to the tertiary.

This transition was recognized by Mr. Gabb, and yet he referred the whole series to the cretaceous. His view was that, a portion of the series being assigned to the cretaceous, the remainder of it must follow, because the series can only be arbitrarily divided; and other geologists still entertain a similar opinion. By whatever name or names this great series of strata may be known, it is plain that it represents a continuous portion of geological time, extending from the later mesozoic to the earlier cenozoic age inclusive. Therefore the mesozoic series of strata in this portion of the Pacific coast region has really no definable upper limit.

It is true that by our present methods it is inconvenient to classify a series of strata like this, but the recognition of its true character is of far more importance than mere convenience of classification. Indeed this case constitutes one of the most instructive

discoveries that has been made in the whole range of historical geology; and it should be understood as demonstrating that abrupt transitions from one epoch, period, or age to another have always been due to local or regional changes in physical conditions; or, in other words, to accidental circumstances.

Concerning the relation of the other members of the California section of the mesozoic to the Chico-Téjon series, or to each other, and the relation of the lowest of those formations to the Jurassic, our knowledge, as before mentioned, is imperfect.

The satisfactory correlation of a part of the cretaceous formations of the interior region with those of the Atlantic coast region has already been mentioned; but we have never been able to satisfactorily correlate any of the cretaceous formations of the Pacific coast region which have been mentioned, with any of those of the interior and Atlantic coast regions, even in cases of presumable contemporaneity. If such correlations are ever made, we must expect them through the labors of the Canadian geologists in the North-west. The whole fauna of each of the Pacific coast formations referred to seems to be different from that of any of the more eastern formations, the few cases in which specific identity has been recognized being of doubtful character. This inability to correlate formations in different and not far distant parts of our own continent, which were presumably contemporaneous in their origin, may well cause us to doubt the correlation of at least a part of the American formations with those of other parts of the world which various authors have confidently assumed.

It has already been shown that the lower limit of the North American mesozoic must coincide with the lowermost triassic strata in any given section, whether those strata are regarded as representing the earlier or the later trias; and that no strata hitherto recognized as Permian can be reasonably referred to the mesozoic. That is, the lower limit is defined by a great break in the geological record of this continent, constituting an hiatus, which began before the full completion of paleozoic time and continued until after the beginning of mesozoic time.

But we are quite unable to designate clearly the upper limit of the mesozoic in at least a large portion of this continent. It is true that in the Atlantic coast region the upper limit of the mesozoic is clearly marked where the marine eocene rests upon the uppermost of the cretaceous strata there, but that delimitation is produced by an hiatus. In portions of both the interior and Pacific coast regions, however, it is quite impossible to clearly designate the delimitating boundary between the mesozoic and cenozoic, because in at least a part of both regions no break in either the stratigraphical or paleontological record occurred until after cenozoic time was fully established.

In connection with the foregoing brief summary of the characteristics of the North American mesozoic, certain views have been expressed which I entertain in common with some, but not all, other geologists concerning the correlation of formations and the inter-relation of presumably contemporaneous fossil faunas and floras. The following propositions are offered as the basis of those views. A part of them, however, will not be questioned by any geologist, but these are given with the others for the sake of relevancy.

(1) In accordance with the principles of modern biology, we must conclude that, although it has not been demonstrated by actual discovery, there has been a continuous genetic succession of living organisms upon the earth ever since life began; that is, while numerous breaks in that succession have occurred, they have never been of universal, but only of local or regional extent, and they have been due to similarly restricted physical changes.

(2) The record of that succession of living organisms has been accomplished and preserved by the natural entombment of their fossilizable remains in aqueous sedimentary deposits. Subsequent physical changes have destroyed or rendered inaccessible a large part of the record, and all we know of that succession is derived from such of those remains as we have been fortunate enough to discover.

(3) The record of the succession of terrestrial life has been far less complete, and has suffered greater interruptions, than that of aqueous life, because the record of the former has been made under conditions which were irrelevant or inimical to that life, and

the entombment of its remains has always occurred under accidental conditions.

(4) The record of marine life is necessarily more complete than that of any other, because the seas have furnished continuous and more uniform conditions than either the land or fresh waters, and because the preservation of its remains was a natural consequence of the conditions under which that life existed. Therefore the record of marine life was less modified by other than evolutionary changes of a cosmical character than that of the land and fresh waters, and it is consequently more trustworthy as an index of the progress of geological time.

(5) Breaks or interruptions in the succession of marine forms of life have been coincident with breaks of continuity, or with changes in the characters of the sediments by which their remains were entombed. These breaks in sedimentation, and in the succession of living organisms, are used by all geologists as indicating the delimiting boundaries of geological epochs, periods, and ages respectively, as well as of formations and systems. Their causes were independent of the existence of life, and their occurrence was accidental with reference to it.

It therefore follows that the recognizable time record in one part of the world is necessarily different in its divisions from that of any other part. For example, a period the close of which was marked by such interruptions as have been mentioned in one part of the world would be continued in other parts as long afterward as the occurrence of similar breaks there should be postponed. While such interruptions were occurring in one or more parts of the world, life and sedimentation were continuous and unaffected by them in others. This is plainly shown in the case of the Chico-Téjon series in California, because no inter-delimiting boundary occurs between its cretaceous and the tertiary portions, as has already been explained; while an evident hiatus exists between the uppermost known cretaceous and the lowermost known tertiary both in Europe and a large part of North America.

(6) While there has been progressive development in the order of succession of living organisms from lower forms in earlier, to higher forms in later geological time, the rate of progress of that development has not been uniform in all parts of the world for the same kinds of life. For example, the plant life of North America is now understood to have reached, in later mesozoic time, a higher stage of development with relation to animal life than it had in Europe; and the difference in grade among the now living indigenous faunas of the different continents respectively, indicates that a similar difference in the rate of development has also prevailed in different divisions of the animal kingdom.

(7) The various stages of progressive development of living organisms have been marked by the successive introduction and extinction of class, ordinal, family, and generic types; and yet certain of those types survived in some parts of the world during long epochs after they had become extinct in other parts. This proposition is supported by such facts as that of the survival into the Laramie, Arapahoe, and Denver epochs, of dinosaurian faunas which apparently show little if any indication of decadence or of approaching extinction; and also by the survival of highly organized representatives of mesozoic families and genera to the present time. Therefore it is not to be expected that we should find exactly the same association of faunal and floral types, or evidence of more than approximately the same grade of development of life in contemporaneous but widely separated formations. Therefore, also, the custom which has been adopted by some paleontologists of making the assumed absence of certain of those types a distinguishing element in the chronological diagnosis of formations is by no means to be commended, even if it were possible for us to discover remains of all the forms of life which then and there existed.

(8) Correlation of lake and inland sea deposits with those of open-sea origin, even within the same continental area, is necessarily a matter of uncertainty. This uncertainty is due to the great difference in the character of the faunas of those waters respectively, to the fact that constituent members of faunas of inland waters were not so diversely differentiated in the course of geological time as were those of marine waters; and also the inevitable want of geographical continuity of the two classes of deposits with each other, even in cases of actual contemporaneity. The only really

trustworthy paleontological means of determining the equivalency or contemporaneity of deposits in such cases as these is the specific identification of such remains of land animals and plants as may have found entombment in then existing contiguous inland waters, on the one hand, and marine waters on the other. For reasons mentioned in proposition 6, the mere similarity of types, even of the more highly organized animals and plants, which may be discovered in different districts cannot be relied upon as indicating contemporaneity. Geographical continuity of strata being always wanting in such cases, the only aid to be expected from stratigraphy in determining equivalency of the formations must come through the discovery of the overlying or underlying position of the inland deposits with reference to marine deposits of known geological age.

It will be seen that these propositions involve serious questionings of the validity of certain methods and practices common among many of those geologists who devote themselves mainly or exclusively to paleontology. Such questionings afford scope for elaborate and varied discussions, but I shall close my present remarks with only a brief reference to the general subject of a proper recognition of a universal scheme of geological classification, which must of course have a biological basis.

The greater part of my own geological studies having been prosecuted from a biological standpoint, I am naturally not disposed to underestimate the value of paleontology as a branch of geological investigation, nor to encourage, even by incidental utterance, those who do. But I am sure no greater harm can be done to paleontological science than either to encourage, or to fail to oppose, the erroneous views which some of its votaries are shown by their own publications to entertain. For example, it is apparent to every one who is at all familiar with paleontological literature that many authors assume to designate with precision the geological age of any and all fossils submitted to them, as well as the taxonomic position of the strata from which they were obtained, without reference to stratigraphy, or to any related geological fact.

Those paleontologists who make this unwarranted application of their science to systematic geology, all use the scheme of classification that has been established for Europe, and use it as if it were of infallible application to all other parts of the world, and also as if it were already absolutely perfected for that continent. While I have no inclination to question the general accuracy of the European scheme of classification for that continent, I do not hesitate to express the opinion that it is not of infallible application to other parts of the world, except as to its larger divisions, and that even in this respect it will need modification. That is, I hold that investigations of the formations which are found upon any given continent or great division of the earth's surface ought to be prosecuted, first, with relation to one another, and second, with reference to their ultimate, not immediate, correlation with those of other continents or divisions.

It is true that the general consensus of geological thought and opinion has long been in favor of adopting the European scheme of classification in all, or nearly all, its details as applicable to all other parts of the world, and every considerate naturalist will treat such opinion with deference. But prevalence of opinion is by no means proof of its accuracy. None of the older naturalists present need be reminded of the great revolution in opinion that took place a little more than twenty years ago; and the older geologists will remember that the degree of displacement, the amount of consolidation, the crystallization and the lithological composition, of strata, were once accepted by all geologists as indices of the geological age of the formations which they composed. Remembering these incidents in the history of natural science, it does not seem unreasonable that present opinions should be frequently questioned, even those which are generally accepted.

I do not wish to be understood as condemning the scheme of classification now in use, nor even as recommending the present substitution of it by any other; but I insist that for universal application, it is plainly imperfect. A scheme of classification, as a working rule, is not only a convenience but a constant necessity; so constant, indeed, that I have not been able to present these remarks without its aid. But while the one which has been established for Europe ought by no means to be discarded, it ought to

be used tentatively in each of the great divisions of the earth, and with reference to the ultimate establishment of a universal scheme after all those divisions have been thoroughly investigated.

The time has come when North American geologists can, and ought to, hold a commanding position in this respect; and when we have elaborated a scheme of classification for the formations of our own continent, it will have equal claim to the favorable consideration of the geological world with any other.

NOTES AND NEWS.

AFTER a stoppage of two years, caused by a lack of funds, work was recently resumed on the double tunnel under the Hudson River between this city and the New Jersey side. Operations are restricted as yet to the Jersey City end of the north or up-river tunnel, which has been excavated to a distance of nearly two thousand feet from the shaft. The total length of the tunnel from shaft to shaft, when completed, will be 5,600 feet, to which must be added the length of the inclines or approaches leading to the surface, work upon which has not been begun. Work is carried on under an air-pressure of about thirty-four pounds to the square inch, and the heading progresses at the rate of twenty-five feet a week.

— Professor Elihu Thompson has perfected an invention by which the rails of street or steam railways may be welded together by electricity after being placed in position. A dynamo propels over the tracks an electric welding machine, which welds the rails into one continuous line after it passes over them. It is proposed to have at every one hundred feet a break, to allow for expansion. Any kind of rails can thus be welded.

— There has been patented in Germany a process by means of which sulphuric acid for manufacturing purposes can be safely transported. The inventor takes advantage of a property of certain salts—of which alkaline sulphates are representatives—by which they give up their water of crystallization when heated and take it up again when cool; and he does so by mixing the salts in an anhydrous condition with a calculated quantity of sulphuric acid. The whole mass becomes granular, or may be formed into cakes, and when heated the whole liquefies, and may be used as if it were sulphuric acid, for the presence of bisulphate of soda does no harm.

— Several reports received at the Hydrographic Office in Washington during the past month serve to illustrate the source of many doubtful or imaginary dangers to navigation that encumber the charts so long before their existence can be disproved. On July 14, in 43° 17' north latitude, 57° 32' west longitude, the captain of a Norwegian vessel sighted an immense dead whale which at a distance had the appearance of a rock. A number of sea-birds were about it. On July 22 the German steamship "National," while on a scientific exploring expedition, passed a dead whale under similar circumstances. On Aug. 2 the captain of a British steamship sighted a dead whale, about a hundred feet long, showing six feet out of water. It will readily be seen how easily such an obstruction might be mistaken for a shoal, and, if reported in a region where the depths are not too well known to admit of the possibility of such a thing, it might add one more doubtful danger to the many that have been reported.

— A nailless horseshoe which has been undergoing severe tests in England during the past two years, with satisfactory results, is described as follows: The shoe is attached by a steel band which passes below the coronet from one extremity of the heel to the other. This band is kept in position by a steel pillar which runs from the centre of the shoe up to the centre of the hoof. In addition there are three short studs, one in the centre of the shoe, and the others near the heel and on each side of it. It can be put on by any one who has once seen the process, which takes about half the time required with the cold-shoe system, which latter is an improvement as regards time on the ordinary process with nails. The nailless shoe diminishes or puts an end to cutting, and is particularly suited to brittle hoofs or hoofs with sand cracks. It costs as little, weighs as little, and lasts as long as the ordinary shoe; and, moreover, is not sucked off on heavy ground.

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ONE OF THE FEATURES of the meeting of The American Association at Toronto just closed was the growth of the societies devoted to special branches of science which meet each year at the same time and place as the association. The Botanical Club has been in successful operation for some years, as has also the Agricultural Society. This year there was held a meeting of the Geologic Society, and the formation of a chemical society was vigorously discussed. The Entomological Club is another of the groups into which congenial spirits unite, possibly to free themselves of the more cumbersome meetings of the sections of the association. Of the vice-presidents' addresses, we print this week those of vice-presidents R. S. Woodward and C. A. White. That by Professor H. S. Carhart, in the Physics Section, was a review of theories of electrical action. In the Chemical Section, Professor W. L. Dudley spoke of "The Nature of Amalgams." He gave a *résumé* of the most important work done in this interesting field, and a few results of his own. Appended to the address is a complete index to the literature, embracing over three hundred titles. In the Section of Mechanics and Engineering no address was delivered, and the work of the section was quickly over, few papers being presented. Vice-president G. L. Goodale's address before the Biologic Section was on protoplasm. The address of Vice-president Garrick Mallery, before the Anthropologists, treated of the "Israelite and Indian, a Parallel in Planes of Culture." This we hope to print in an early number. The remarks of Vice-president C. S. Hill before the Eco-

nomic Section on the "Economic and Sociologic Relations of the Canadian States and the United States, prospectively considered," roused considerable criticism. The meeting adjourned to meet next year on the third Wednesday in August at Indianapolis. The officers of the meeting will be as follows: president, Professor George L. Goodale, Harvard University; vice-presidents, A, Mathematics and Astronomy, S. C. Chandler, Cambridge, Mass.; B, Physics, Cleveland Abbe, Washington; C, Chemistry, R. B. Warder, Washington; D, Mechanical Science and Engineering, James E. Denton, Hoboken, N. J.; E, Geology and Geography, John C. Branner, Little Rock, Ark.; F, Biology, C. S. Minot, Boston, Mass.; H, Anthropology, Frank Baker, Washington; I, Economic Science and Statistics, J. Richards Dodge, Washington; permanent secretary, F. W. Putnam, Cambridge, Mass., office, Salem, Mass.; general secretary, H. Carrington Bolton, of New York; secretary of the council, James Loudon, Toronto; secretaries of the sections, A, Wooster A. Beman, Ann Arbor, Mich.; B, W. Le Conte Stevens, Brooklyn, N. Y.; C, W. A. Noyes, Terre Haute, Ind.; D, M. E. Cooley, Ann Arbor, Mich.; E, Samuel Calvin, Iowa City, Iowa; F, John M. Coulter, Crawfordsville, Ind.; H, Joseph Jastrow, Madison, Wis.; I, S. Dana Horton, Pomeroy, Ohio; treasurer, William Lilly, of Mauch Chunk, Penn.; auditors, Henry Wheatland, Salem, Mass.; Thomas Meehan, Philadelphia.

THE MATHEMATICAL THEORIES OF THE EARTH.¹

THE name of this section, which, by your courtesy, it is my duty to address to-day, implies a community of interest among astronomers and mathematicians. This community of interest is not difficult to explain. We can of course imagine a considerable body of astronomical facts quite independent of mathematics. We can also imagine a much larger body of mathematical facts quite independent of and isolated from astronomy. But we never think of astronomy in the large sense without recognizing its dependence on mathematics, and we never think of mathematics as a whole without considering its capital applications in astronomy.

Of all the subjects and objects of common interest to us the earth will easily rank first. The earth furnishes us with a stable foundation for instrumental work and a fixed line of reference, whereby it is possible to make out the orderly arrangement and procession of our solar system and to gain some inkling of other systems which lie within telescopic range. The earth furnishes us with a most attractive store of real problems: its shape, its size, its mass, its precession and nutation, its internal heat, its earthquakes and volcanoes, and its origin and destiny, are to be classed with the leading questions for astronomical and mathematical research. We must of course recognize the claims of our friends the geologists to that indefinable something called the earth's crust, but, considered in its entirety and in its relations to similar bodies of the universe, the earth has long been the special province of astronomers and mathematicians. Since the times of Galileo and Kepler and Copernicus it has supplied a perennial stimulus to observation and investigation, and it promises to tax the resources of the ablest observers and analysts for some centuries to come. The mere mention of the names of Newton, Bradley, d'Alembert, Laplace, Fourier, Gauss, and Bessel calls to mind not only a long list of inventions and discoveries, but the most important parts of mathematical literature. In its dynamical and physical aspects the earth was to them the principal object of research, and the thoroughness and completeness of their contributions toward an explanation of the "system of the world" are still a source of wonder and admiration to all who take the trouble to examine their works.

A detailed discussion of the known properties of the earth and of the hypotheses concerning the unknown properties, is no fit task for a summer afternoon: the intricacies and delicacies of the subject are suitable only for another season and a special audience. But it has seemed that a somewhat popular review of the state of our mathematical knowledge of the earth might not be without in-

terest to those already familiar with the complex details, and might also help to increase that general interest in science, the promotion of which is one of the most important functions of this association.

As we look back through the light of modern analysis, it seems strange that the successors of Newton, who took up the problem of the shape of the earth, should have divided into hostile camps over the question whether our planet is elongated or flattened at the poles. They agreed in the opinion that the earth is a spheroid, but they debated, investigated, and observed for nearly half a century before deciding that the spheroid is oblate rather than oblong. This was a critical question, and its decision marks perhaps the most important epoch in the history of the figure of the earth. The Newtonian view of the oblate form found its ablest supporters in Huyghens, Maupertuis, and Clairaut, while the erroneous view was maintained with great vigor by the justly distinguished Cassinian school of astronomers. Unfortunately for the Cassinians, defective measures of a meridional arc in France gave color to the false theory and furnished one of the most conspicuous instances of the deterring effect of an incorrect observation. As you well know, the point was definitely settled by Maupertuis's measurement of the Lapland arc. For this achievement his name has become famous in literature as well as in science, for his friend Voltaire congratulated him on having "flattened the poles and the Cassinis," and Carlyle has honored him with the title of "Earth-flattener."

Since the settlement of the question of the form, progress towards a knowledge of the size of the earth has been consistent and steady, until now it may be said that there are few objects with which we have to deal whose dimensions are so well known as the dimensions of the earth. But this is a popular statement, and like most such, needs to be explained in order not to be misunderstood. Both the size and shape of the earth are defined by the lengths of its equatorial and polar axes; and, knowing the fact of the oblate spheroidal form, the lengths of the axes may be found within narrow limits from simple measurements conducted on the surface, quite independently of any knowledge of the interior constitution of the earth. It is evident in fact, without recourse to mathematical details, that the length of any arc, as a degree of latitude or longitude, on the earth's surface, must depend on the lengths of those axes. Conversely, it is plain that the measurement of such an arc on the surface and the determination of its geographical position, constitute an indirect measurement of the axes. Hence it has happened that scientific as distinguished from practical geodesy has been concerned chiefly with such linear and astronomical measurements, and the zeal with which this work has been pursued is attested by triangulations on every continent.

Passing over the earlier determinations as of historical interest only, all of the really trustworthy approximations to the lengths of the axes have been made within the half century just passed. The first to appear of these approximations were the well-founded values of Airy, published in 1830. These, however, were almost wholly overshadowed and supplanted eleven years later by the values of Bessel, whose spheroid came to occupy a most conspicuous place in geodesy for more than a quarter of a century. Knowing as we now do that Bessel's values were considerably in error, it seems not a little remarkable that they should have been so long accepted without serious question. One obvious reason is found in the fact that a considerable lapse of time was essential for the accumulation of new data, but two other possible reasons of a different character are worthy of notice, because they are interesting and instructive whether specially applied to this particular case or not. It seems not improbable that the close agreement of the values of Airy and Bessel, computed independently and by different methods, — the greatest discrepancy being about one hundred and fifty feet, — may have been incautiously interpreted as a confirmation of Bessel's dimensions, and hence led to their too ready adoption. It seems also not improbable that the weight of Bessel's great name may have been too closely associated in the minds of his followers with the weight of his observations and results. The sanction of eminent authority, especially if there is added to it the stamp of an official seal, is sometimes a serious obstacle to real progress. We cannot do less than accord to Bessel the first place among the

¹ Address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science, at Toronto, Ont., Aug. 28-Sept. 3, by R. S. Woodward, vice-president of the section.

astronomers and geodesists of his day, but this is no adequate justification for the exaggerated estimate long entertained of the precision of the elements of his spheroid.

The next step in the approximation was the important one of Clarke in 1866. His new values showed an increase over Bessel's of about half a mile in the equatorial semi-axis and about three-tenths of a mile in the polar semi-axis. Since 1886, General Clarke has kept pace with the accumulating data, and given us so many different elements for our spheroid that it is necessary to affix a date to any of his values we may use. The later values, however, differ but slightly from the earlier ones, so that the spheroid of 1866, which has come to be pretty generally adopted, seems likely to enjoy a justly greater celebrity than that of its immediate predecessor. The probable error of the axes of this spheroid is not much greater than the hundred thousandth part, and it is not likely that new data will change their lengths by more than a few hundred feet.

In the present state of science, therefore, it may be said that the first order of approximation to the form and dimensions of the earth has been successfully attained. The question which follows naturally and immediately is, how much further can the approximation be carried? The answer to this question is not yet written, and the indications are not favorable for its speedy announcement. The first approximation, as we have seen, requires no knowledge of the interior density and arrangement of the earth's mass; it proceeds on the simple assumption that the sea surface is closely spheroidal. The second approximation, if it be more than a mere interpolation formula, requires a knowledge of both the density and arrangement of the constituents of the earth's mass, and especially of that part called the crust. "All astronomy," says Laplace, "rests on the stability of the earth's axis of rotation." In a similar sense we may say all geodesy rests on the direction of the plumb-line. The simple hypothesis of a spheroidal form assumes that the plumb-line is everywhere coincident with the normal to the spheroid, or that the surface of the spheroid coincides with the level of the sea. But this is not quite correct. The plumb-line is not in general coincident with the normal, and the actual sea level or geoid must be imagined to be an irregular surface lying partly above and partly below the ideal spheroidal surface. The deviations, it is true, are relatively small, but they are in general much greater than the unavoidable errors of observation, and they are the exact numerical expression of our ignorance in this branch of geodesy. It is well known, of course, that deflections of the plumb-line can sometimes be accounted for by visible masses, but on the whole it must be admitted that we possess only the vaguest notions of their cause and a most inadequate knowledge of distribution and extent.

What is true of plumb-line deflections is about equally true of the deviations of the intensity of gravity from what may be called the spheroidal type. Given a closely spheroidal form of the sea level and it follows from the law of gravitation, as a first approximation, without any knowledge of the distribution of the earth's mass, that the increase of gravity varies as the square of the sine of the latitude in passing from the equator to the poles. This is the remarkable theorem of Stokes, and it enables us to determine the form or ellipticity of the earth, by means of pendulum observations alone. It must be admitted, however, that the values for the ellipticity recently obtained in this way by the highest authorities, Clarke and Helmert, are far from satisfactory, whether we regard them in the light of their discrepancy or in the light of the different methods of computing them. In general terms we may say that the difficulty in the way of the use of pendulum observations still hinges on the treatment of local anomalies and on the question of reduction to sea level. At present, the case is one concerning which the doctors agree neither in their diagnosis nor in their remedies.

Turning attention now from the surface, towards the interior, what can be said of the earth's mass as a whole, of its laws of distribution, and of the pressures that exist at great depth? Two facts, namely, the mean density and the surface density, are roughly known; and a third fact, namely, the precession constant, or the ratio of the difference of the two principal moments of inertia to the greater of them, is known with something like pre-

cision. These facts lie within the domain of observation, and require only the law of gravitation for their verification. Certain inferences also from these facts and others have long been and still are held to be hardly less cogent and trustworthy, but before stating them, it will be well to recall briefly the progress of opinion concerning this general subject during the past century and a half.

The conception of the earth as having been primitively fluid was the prevailing one among mathematicians before Clairaut published his "*Théorie de la Figure de la Terre*" in 1743. By the aid of this conception Clairaut proved the celebrated theorem which bears his name, and probably no idea in the mechanics of the earth has been more suggestive and fruitful. It was the central idea in the elaborate investigations of Laplace, and received at his hands a development which his successors have found it about equally difficult to displace or to improve. From the idea of fluidity spring naturally the hydrostatical notions of pressure and level surfaces, or the arrangement of fluid masses in strata of uniform density. Hence follows, also, the notion of continuity of increase in density from the surface towards the centre of the earth. All of the principal mechanical properties and effects of the earth's mass, viz., the ellipticity, the surface density, the mean density, the precession constant, and the lunar inequalities, were correlated by Laplace in a single hypothesis, involving only one assumption in addition to that of original fluidity and the law of gravitation. This assumption relates to the compressibility of matter, and asserts that the ratio of the increment of pressure to the increment of density is proportional to the density. Many interesting and striking conclusions follow readily from this hypothesis, but the most interesting and important are those relative to density and pressure, especially the latter, whose dominance as a factor in the mechanics of celestial masses seems destined to survive whether the hypothesis stands or falls. The hypothesis requires that while the density increases slowly from something less than 3 at the surface to about 11 at the centre of the earth, the pressure within the mass increases rapidly below the surface, reaching a value surpassing the crushing strength of steel at the depth of a few miles, and amounting at the centre to no less than three million atmospheres. The inferences, then, as distinguished from the facts, are that the mass of the earth is very nearly symmetrically disposed about its centre of gravity, that pressure and density except near the surface are mutually dependent, and that the earth in reaching this stage has passed through the fluid or quasi-fluid state.

Later writers have suggested other hypotheses for a continuous distribution of the earth's mass, but none of them can be said to rival the hypothesis of Laplace. Their defects lie either in not postulating a direct connection between density and pressure or in postulating a connection which implies extreme or impossible values for these and other mechanical properties of the mass.

It is clear from the positiveness of his language in frequent allusions to this conception of the earth, that Laplace was deeply impressed with its essential correctness. "Observations," he says, "prove incontestably that the densities of the strata [*couches*] of the terrestrial spheroid increase from the surface to the centre;" and "the regularity with which the observed variation in length of a seconds pendulum follows the law of the squares of the sines of the latitudes, proves that the strata are arranged symmetrically about the centre of gravity of the earth." The more recent investigations of Stokes, to which allusion has already been made, forbid our entertaining anything like so confident an opinion of the earth's primitive fluidity or of a symmetrical and continuous arrangement of its strata. But, though it must be said that the sufficiency of Laplace's arguments has been seriously impugned, we can hardly think the probability of the correctness of his conclusions has been proportionately diminished.

Suppose, however, that we reject the idea of original fluidity. Would not a rotating mass of the size of the earth assume finally the same aspects and properties presented by our planet? Would not pressure and centrifugal force suffice to bring about a central condensation and a symmetrical arrangement of strata similar at least to that required by the Laplacian hypothesis? Categorical answers to these questions cannot be given. But whatever may have been the antecedent condition of the earth's mass, the conclusion seems unavoidable that at no great depth the pressure is suffi-

cient to break down the structural characteristics of all known substances, and hence to produce viscous flow whenever and wherever the stress difference exceeds a certain limit, which cannot be large in comparison with the pressure: Purely observational evidence also of a highly affirmative kind in support of this conclusion, is afforded by the remarkable results of Tresca's experiments on the flow of solids and by the abundant proofs in geology of the plastic movements and viscous flow of rocks. With such views and facts in mind, the fluid stage, considered indispensable by Laplace, does not appear necessary to the evolution of a planet, even if it reach the extreme refinement of a close fulfilment of some such mathematical law as that of his hypotheses. If, as is here assumed, pressure be the dominant factor in such large masses, the attainment of a stable distribution would be simply a question of time. The fluid mass might take on its normal form in a few days or a few months, whereas the viscous mass might require a few thousand or a few million years.

Some physicists and mathematicians, on the other hand, reject both the idea of the existence of great pressures within the earth's mass, and the notion of an approach to continuity in the distribution of density. As representing this side of the question, the views of the late M. Roche, who wrote much on the constitution of the earth, are worthy of consideration. He tells us that the very magnitude of the central pressure computed on the hypothesis of fluidity is itself a peremptory objection to that hypothesis. According to his conception, the strata of the earth from the centre outwards are substantially self-supporting and unyielding. It does not appear, however, that he had submitted this conception to the test of numbers, for a simple calculation will show that no materials of which we have any knowledge would sustain the stress in such shells or domes. If the crust of the earth were self-supporting, its crushing strength would have to be about thirty times that of the best cast steel or five to one thousand times that of granite. The views of Roche on the distribution of terrestrial densities appear equally extreme. He prefers to consider the mass as made up of two distinct parts, an outer shell or crust whose thickness is about one-sixth of the earth's radius, and a solid nucleus having little or no central condensation. The nucleus is conceived to be purely metallic, and to have about the same density as iron. To account for geological phenomena, he postulates a zone of fusion separating the crust from the nucleus. The whole hypothesis is consistently worked out in conformity with the requirements of ellipticity, the superficial density, the mean density, and precession; so that to one who can divest his mind of the notion that pressure and continuity are important factors in the mechanics of such masses, the picture which Roche draws of the constitution of our planet will present nothing incongruous.

In a field so little explored and so inaccessible, though hedged about as we have seen by certain sharply limiting conditions, there is room for a wide range of opinion and for great freedom in the play of hypothesis; and although the preponderance of evidence appears to be in favor of a terrestrial mass in which the reign of pressure is well-nigh absolute, we should not be surprised a few decades or centuries hence to find many of our notions on this subject radically defective.

If the problem of the constitution and distribution of the earth's mass is yet an obscure and difficult one after two centuries of observation and investigation, can we report any greater degree of success in the treatment of that still older problem of the earth's internal heat, of its origin and effects? Concerning phenomena always so impressive and often so terribly destructive as those intimately connected with the terrestrial store of heat, it is natural that there should be a considerable variety of opinion. The consensus of such opinion, however, has long been in favor of the hypothesis that heat is the active cause of many and a potent factor in most of the grander phenomena which geologists assign to the earth's crust; and the prevailing interpretation of these phenomena is based on the assumption that our planet is a cooling sphere whose outer shell or crust is constantly cracked and crumpled in adjusting itself to the shrinking nucleus.

The conception that the earth was originally an intensely heated and molten mass appears to have first taken something like definite form in the minds of Leibnitz and Descartes. But neither of

these philosophers was armed with the necessary mathematical equipment to subject this conception to the test of numerical calculation. Indeed it was not fashionable in their day, any more than it is with some philosophers in ours, to undertake the drudgery of applying the machinery of analysis to the details of an hypothesis. Nearly a century elapsed before an order of intellects capable of dealing with this class of questions appeared. It was reserved for Joseph Fourier to lay the foundation and build a great part of the superstructure of our modern theory of heat diffusion, his avowed desire being to solve the great problem of terrestrial heat. "The question of terrestrial temperatures," he says, "has always appeared to us one of the grandest objects of cosmological studies, and we have had it constantly in view in establishing the mathematical theory of heat." This ambition, however, was only partly realized. Probably Fourier underestimated the difficulties of his problem, for his most ingenious and industrious successors in the same field have made little progress beyond the limits he attained. But the work he left is a perennial index to his genius. Though quite inadequately appreciated by his contemporaries, the "Analytical Theory of Heat," which appeared in 1820, is now conceded to be one of the epoch-making books. Indeed, to one who has caught the spirit of the extraordinary analysis which Fourier developed and illustrated by numerous applications in this treatise, it is evident that he opened a field whose resources are still far from being exhausted. A little later Poisson took up the same class of questions and published another great work on the mathematical theory of heat. Poisson narrowly missed being the foremost mathematician of his day. In originality, in wealth of mathematical resources, and in breadth of grasp of physical principles, he was the peer of the ablest of his contemporaries. In lucidity of exposition it would be enough to say that he was a Frenchman, but he seems to have excelled in this peculiarly national trait. His contributions to the theory of heat have been somewhat overshadowed in recent times by the earlier and perhaps more brilliant researches of Fourier, but no student can afford to take up that enticing though difficult theory without the aid of Poisson as well as Fourier.

It is natural, therefore, that we should inquire what opinions these great masters in the mathematics of heat diffusion held concerning the earth's store of heat. I say "opinions," for, unhappily, this whole subject is still so largely a matter of opinion that in discussing it one may not inappropriately adopt the famous caution of Marcus Aurelius,—"Remember that all is opinion." It does not appear that Fourier reached any definite conclusion on this question, though he seems to have favored the view that the earth in cooling from an earlier state of incandescence reached finally, through convection, a condition in which there was a uniform distribution of heat throughout its mass. This is the *consistentior status* of Leibnitz, and it begins with the formation of the earth's crust if not with the consolidation of the entire mass. It thus affords an initial distribution of heat and an epoch from which analysis may start, and the problem for the mathematician is to assign the subsequent distribution of heat and the resulting mechanical effects. But no great amount of reflection is necessary to convince one that the analysis cannot proceed without making a few more assumptions. The assumptions which involve the least difficulty, and which for this reason partly have met with most favor, are that the conductivity and thermal capacity of the entire mass remain constant, and that the heat conducted to the surface of the earth passes off by the combined process of radiation, convection, and conduction, without producing any sensible effect on surrounding space. These or similar assumptions must be made before the application of theory can begin. In addition, two data are essential to numerical calculations, namely, the diffusivity, or the ratio of the conductivity of the mass to its thermal capacity, and the initial uniform temperature. The first of these can be observed, approximately at least; the second can only be estimated at present. With respect to these important points which must be considered after the adoption of the *consistentior status*, the writings of Fourier afford little light. He was content, perhaps, to invent and develop the exquisite analysis requisite to the treatment of such problems.

Poisson wrote much on the whole subject of terrestrial temper-

atures, and carefully considered most of the troublesome details which lay between his theory and its application. While he admitted the nebular hypothesis and an initial fluid state of the earth, he rejected the notion that the observed increase of underground temperature is due to a primitive store of heat. If the earth was originally fluid by reason of its heat, a supposition which Poisson regarded quite gratuitous, he conceived that it must cool and consolidate from the centre outwards; so that according to this view the crust of our planet arrived at a condition of stability only after the supply of heat had been exhausted. But Poisson was not at a loss to account for the observed temperature gradient in the earth's crust. Always fertile in hypotheses, he advanced the idea that there exist, by reason of interstellar radiations, great variations in the temperature of space, some vast regions being comparatively cool and others intensely hot, and that the present store of terrestrial heat was acquired by a journey of the solar system through one of the hotter regions. "Such is," he says, "in my opinion, the true cause of the augmentation of temperature which occurs as we descend below the surface of the globe." This hypothesis was the result of Poisson's mature reflection, and as such is well worthy of attention. The notion that there exist hot foci in space was advanced also in another form in 1852 by Rankine, in his interesting speculation on the re-concentration of energy. But whatever we may think of the hypothesis as a whole, it does not appear to be adequate to the case of the earth unless we suppose the epoch of transit through the hot region exceedingly remote and the temperature of that region exceedingly high. The continuity of geological and paleontological phenomena is much better satisfied by the Leibnitzian view of an earth long subject to comparatively constant surface conditions but still active with the energy of its primitive heat.

Notwithstanding the indefatigable and admirable labors of Fourier and Poisson in this field, it must be admitted that they accomplished little more than the preparation of the machinery with which their successors have sought and are still seeking to reap the harvest. The difficulties which lay in their way were not mathematical but physical. Had they been able to make out the true conditions of the earth's store of heat, they would undoubtedly have reached a high grade of perfection in the treatment of the problem. The theory as they left it was much in advance of observation, and the labors of their successors have therefore necessarily been directed largely towards the determination of the thermal properties of the earth's crust and mass.

Of those who in the present generation have contributed to our knowledge and stimulated the investigation of this subject, it is hardly necessary to say that we owe most to Sir William Thomson. He has made the question of terrestrial temperatures highly attractive and instructive to astronomers and mathematicians, and not less warmly interesting to geologists and paleontologists. Whether we are prepared to accept his conclusions or not, we must all acknowledge our indebtedness to the contributions of his master hand in this field as well as in most other fields of terrestrial physics. The contribution of special interest to us in this connection is his remarkable memoir on the secular cooling of the earth. In this memoir he adopts the simple hypothesis of a solid sphere whose thermal properties remain invariable while it cools by conduction from an initial state of uniform temperature, and draws therefrom certain striking limitations on geologic time. Many geologists were startled by these limitations, and geologic thought and opinion have since been widely influenced by them. It will be of interest, therefore, to state a little more fully and clearly the grounds from which his arguments proceed. Conceive a sphere having a uniform temperature initially, to cool in a medium which instantly dissipates all heat brought by conduction to its surface, thus keeping the surface at a constant temperature. Suppose we have given the initial excess of the sphere's temperature over that of the medium. Suppose also that the capacity of the mass of the sphere for diffusion of heat is known, and known to remain invariable during the process of cooling. This capacity is called diffusivity, and is a constant which can be observed. Then from these data the distribution of temperature at any future time can be assigned, and hence also the rate of temperature increase, or the temperature gradient, from the surface towards the centre of the sphere can be computed. It is tolerably certain that the

heat conducted from the interior to the surface of the earth does not set up any reaction which in any sensible degree retards the process of cooling. It escapes so freely that, for practical purposes, we may say it is instantly dissipated. Hence if we can assume that the earth had a specified uniform temperature at the initial epoch, and can assume its diffusivity to remain constant, the whole history of cooling is known as soon as we determine the diffusivity and the temperature gradient at any point. Now Sir William Thomson determined a value for the diffusivity from measurements of the seasonal variations of underground temperatures, and numerous observations of the increase of temperature with depth below the earth's surface gave an average value for the temperature gradient. From these elements and from an assumed initial temperature of $7,000^{\circ}$, he infers that geologic time is limited to something between twenty million and four hundred million years. He says: "We must allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation cannot have taken place less than twenty million years ago, or we should have more underground heat than we actually have, nor more than four hundred million years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of emergence of the *consistentior status* was probably between those dates." These conclusions were announced twenty-seven years ago, and were republished without modification in 1883.

Recently, also, Professor Tait, reasoning from the same basis, has insisted with equal confidence on cutting down the upper limit of geologic time to some such figures as ten million or fifteen million years. As mathematicians and astronomers, we must all confess to a deep interest in these conclusions and the hypothesis from which they flow. They are very important if true. But what are the probabilities? Having been at some pains to look into this matter, I feel bound to state that, although the hypothesis appears to be the best which can be formulated at present, the odds are against its correctness. Its weak links are the unverified assumptions of an initial uniform temperature and a constant diffusivity. Very likely these are approximations, but of what order we cannot decide. Furthermore, if we accept the hypothesis the odds appear to be against the present attainment of trustworthy numerical results, since the data for calculation obtained mostly from observations on continental areas are far too meagre to give satisfactory average values for the entire mass of the earth. In short, this phase of the case seems to stand about where it did twenty years ago, when Huxley warned us that the perfection of our mathematical mill is no guaranty of the quality of the grist, adding that, "as the grandest mill will not extract wheat-flour from peas-cods, so pages of formulæ will not get a definite result out of loose data."

When we pass from the restricted domain of quantitative results concerning geologic time to the freer domain of qualitative results of a general character, the contractional theory of the earth may be said to still lead all others, though it seems destined to require more or less modification, if not to be relegated to a place of secondary importance. Old as is the notion that the great surface irregularities of the earth are but the outward evidence of a crumpling crust, it is only recently that this notion has been subjected to mathematical analysis on any thing like a rational basis. About three years ago Mr. T. Mellard Reade announced the doctrine that the earth's crust, from the joint effect of its heat and gravitation, should behave in a way somewhat analagous to a bent beam, and should possess at a certain depth a "level of no strain," corresponding to the neutral surface in a beam. Above the level of no strain, according to this doctrine, the strata will be subjected to compression, and will undergo crumpling, while below that level the tendency of the strata to crack and part is overcome by pressure which produces what Reade calls "compressive extension," thus keeping the nucleus compact and continuous. A little later the same idea was worked out independently by Mr. Charles Davison, and it has since received elaborate mathematical treatment at the hands of Darwin, Fisher, and others. The doctrine requires for its application a competent theory of cooling, and hence cannot be depended on at present to give anything better than a general idea of the mechanics of crumpling and a rough estimate of the

magnitudes of the resulting effects. Using Thomson's hypothesis, it appears that the stratum of no strain moves downward from the surface of the earth at a nearly constant rate during the earlier stages of cooling, but more slowly during later stages. Its depth is independent of the initial temperature of the earth; and if we adopt Thomson's value of the diffusivity, it will be about two and a third miles below the surface in a hundred million years from the beginning of cooling, and a little more than fourteen miles below the surface in seven hundred million years. The most important inference from this theory is that the geological effects of secular cooling will be confined for a very long time to a comparatively thin crust. Thus, if the earth is a hundred million years old, crumpling should not extend much deeper than two miles. A test to which the theory has been subjected, and one which some consider crucial against it, is the volumetric amount of crumpling shown by the earth at the present time. This is a difficult quantity to estimate, but it appears to be much greater than the theory alone can account for.

The opponents of the contractional theory of the earth, believing it quantitatively insufficient, have recently revived and elaborated an idea first suggested by Babbage and Herschel in explanation of the greater folds and movements of the crust. This idea figures the crust as being in a state bordering on hydrostatic equilibrium, which cannot be greatly disturbed without a readjustment and consequent movement of the masses involved. According to this view, the transfer of any considerable load from one area to another is followed sooner or later by a depression over the loaded area and a corresponding elevation over the unloaded one; and in a general way it is inferred that the elevation of continental areas tends to keep pace with erosion. The process by which this balance is maintained has been called "isostasy," and the crust is said to be in an isostatic state. The dynamics of the superficial strata with the attendant phenomena of folding and faulting, are thus referred to gravitation alone, or to gravitation and whatever opposing force the rigidity of the strata may offer. In a satisfactory sense, however, the theory of isostasy is in a less satisfactory state than the theory of contraction. As yet we can see only that isostasy is an efficient cause if once set in action; but how it is started and to what extent it is adequate remains to be determined. Moreover, isostasy alone does not seem to meet the requirements of geological continuity, for it tends rapidly towards stable equilibrium, and the crust ought therefore to reach a state of repose early in geologic time. But there is no evidence that such a state has been attained, and but little if any evidence of diminished activity in crustal movements during recent geologic time. Hence we infer that isostasy is competent only on the supposition that it is kept in action by some other cause tending constantly to disturb the equilibrium which would otherwise result. Such a cause is found in secular contraction, and it is not improbable that these two seemingly divergent theories are really supplementary.

Closely related to the questions of secular contraction and the mechanics of crust movements are those vexed questions of earthquakes, volcanism, the liquidity or solidity of the interior, and the rigidity of the earth's mass as a whole,—all questions of the greatest interest but still lingering on the battle-fields of scientific opinion. Many of the "thrice slain" combatants in these contests would fain risk being slain again; and whether our foundation be liquid or solid, or to speak more precisely, whether the earth may not be at once highly plastic under the action of long continued forces and highly rigid under the action of periodic forces of short period, it is pretty certain that some years must elapse before the arguments will be convincing to all concerned. The difficulties appear to be due principally to our profound ignorance of the properties of matter subject to the joint action of great pressure and great heat. The conditions which exist a few miles beneath the surface of the earth are quite beyond the reach of laboratory tests as hitherto developed, but it is not clear how our knowledge is to be improved without resort to experiments of a scale in some degree comparable with the facts to be explained. In the mean time, therefore, we may expect to go on theorizing, adding to the long list of dead theories which mark the progress of scientific thought, with the hope of attaining the truth not so much by direct discovery as by the laborious process of eliminating error.

When we take a more comprehensive view of the problems presented by the earth, and look for light on their solution in theories of cosmogony, the difficulties which beset us are no less numerous and formidable than those encountered along special lines of attack. Much progress has recently been made, however, in the elaboration of such theories. Roche, Darwin, and others have done much to remove the nebulosity of Laplace's nebular hypothesis. Poincaré and Darwin have gone far towards bridging the gaps which have long rendered the theory of rotating fluid masses incomplete. Poincaré has in fact shown us how a homogeneous rotating mass might, through loss of heat and consequent contraction, pass from the spheroidal form to the Jacobian ellipsoidal form, and thence, by reason of its increasing speed of rotation, separate into two unequal masses. Darwin, starting with a swarm of meteorites and gravitation as a basis, has reached many interesting and instructive results in the endeavor to trace out the laws of evolution of a planetary system. But notwithstanding the splendid researches of these and other investigators in this field, it must be said that the real case of the solar system, of the earth and moon, still defies analysis; and that the mechanics of the segregation of a planet from the sun or of a satellite from a planet, if such an event has ever happened, or of the mechanics of the evolution of a solar system from a swarm of meteorites, are still far from being clearly made out.

Time does not permit me to make anything but the briefest allusion to the comparatively new science of mathematical meteorology, with its already considerable list of well-defined theories pressing for acceptance or rejection. Nor need I say more with reference to those older mathematical questions of the tides and terrestrial magnetism than that they are still unsettled. These and many other questions, old and new, might serve equally well to illustrate the principal fact this address has been designed to emphasize, namely, that the mathematical theories of the earth already advanced and elaborated are by no means complete, and that no mathematical Alexander need yet pine for other worlds to conquer.

Speculations concerning the course and progress of science are usually untrustworthy if not altogether fallacious. But, being delegated for the hour to speak to and for mathematicians and astronomers, it may be permissible to offer, in closing, a single suggestion, which will perhaps help us to orient ourselves aright in our various fields of research. If the course of scientific progress in any domain of thought could be drawn, there is every reason to believe that it would exhibit considerable irregularities. There would be marked maxima and minima in its general tendency towards the limit of perfect knowledge; and it seems not improbable that the curve would show throughout some portions of its length a more or less definitely periodic succession of maxima and minima. Races and communities as well as individuals, the armies in pursuit of truth as well as those in pursuit of plunder, have their periods of culminating activity and their periods of placid repose. It is a curious fact that the history of the mathematical theories of the earth presents some such periodicity. We have the marked maximum of the epoch of Newton near the end of the seventeenth century, with the equally marked maximum of the epoch of Laplace near the end of the eighteenth century; and, judging from the recent revival of geodesy and astronomy in Europe, and from the well-nigh general activity in mathematical and geological research, we may hope if not expect that the end of the present century will signalize a similar epoch of productive activity. The minima periods which followed the epochs of Newton and Laplace are less definitely marked but not less noteworthy and instructive. They were not periods of placid repose; to find such one must go back into the night of the middle ages; but they were periods of greatly diminished energy, periods during which those who kept alive the spirit of investigation were almost as conspicuous for their isolation as for their distinguished abilities. Many causes, of course, contributed to produce these minima periods, and it would be an interesting study in philosophic history to trace out the tendency and effect of each cause. It is desired here, however, to call attention to only one cause which contributed to the somewhat general apathy of the periods mentioned, and which always threatens to dampen the ardor of research immediately after the attainment of any marked success or advance. I refer to the im-

pression of contentment with and acquiescence in the results of science, which seems to find easy access to trained as well as untrained minds before an investigation is half completed or even fairly begun. That some such tacit persuasion of the completeness of the knowledge of the earth has at times pervaded scientific thought, there can be no doubt. This was notably the case during the period which followed the remarkable epoch of Laplace. The profound impression of the sufficiency of the brilliant discoveries and advances of that epoch is aptly described by Carlyle in the half humorous, half sarcastic language of Sartor Resartus. "Our Theory of Gravitation," he says, "is as good as perfect: Lagrange, it is well known, has proved that the Planetary System, on this scheme, will endure forever; Laplace, still more cunningly, even guesses that it could not have been made on any other scheme. Whereby, at least, our nautical Logbooks can be better kept; and water transport of all kinds has grown more commodious. Of Geology and Geognosy we know enough: what with the labors of our Werners and Huttons, what with the ardent genius of their disciples, it has come about that now, to many a Royal Society, the creation of a World is little more mysterious than the cooking of a dumpling; concerning which last, indeed, there have been minds to whom the question *How the apples were got in*, presented difficulties." This was written nearly sixty years ago, about the time that the sage of Ecclefechan abandoned his mathematics and astronomy for literature to become the seer of Chelsea, but the force of its irony is still applicable, for we have yet to learn, essentially, "how the apples were got in," and what kind they are.

As to the future, we can only guess, less or more vaguely, from our experience in the past and from our knowledge of present needs. Though the dawn of that future is certainly not heralded by rosy tints of over-confidence amongst those acquainted with the difficulties to be overcome, the prospect, on the whole, has never been more promising. The converging lights of many lines of investigation are now brought to bear on the problems presented by our planet. There is ample reason to suppose that our day will witness a fair average of those happy accidents in science which lead to the discovery of new principles and new methods. We have much to expect from the elaborate machinery and perfected methods of the older and more exact sciences of measuring and weighing — astronomy, geodesy, physics, and chemistry. We have more to expect, perhaps, from geology and meteorology, with their vast accumulations of facts not yet fully correlated. Much, also,

may be anticipated from that new astronomy which looks for the secrets of the earth's origin and history in nebulous masses or in swarms of meteorites. We have the encouraging stimulus of a very general and rapidly growing popular concern in the objects of our inquiries, and the freest avenues for the dissemination of new information; so that we may easily gain the advantage of a concentration of energy without centralization of personal interests. To those, therefore, who can bring the prerequisites of endless patience and unflagging industry, who can bear alike the remorseless discipline of repeated failure and the prosperity of partial success, the field is as wide and as inviting as it ever was to a Newton, or a Laplace.

AMONG THE PUBLISHERS.

"TERMINAL facilities of New York" is the title of the supplement feature in *Harper's Weekly* for Aug. 31. The article is from the pen of Mr. G. T. Ferris, and the illustrations, of which there are thirteen, were drawn by Messrs. Schell and Hogan.

— Following the article on the late Miss Laura Bridgman, in the August *St. Nicholas*, the number for September contains an account of "Helen Keller," the young girl also deaf, dumb and blind, whose rapid advance in her studies was described in *Science* a year ago. The sketch is by Florence Howe Hall, a daughter of Dr. Howe, and contains portraits of the child, of her teacher, a facsimile letter from the little girl herself to Mrs. Hall, and other illustrations. In the same number Lieutenant Hamilton gives a sketch of the modern method of defending coasts or harbors, and shows how necessary such defences have become as a consequence of the development of the world's navies.

— The September number of the *Political Science Quarterly* contains a critical estimate of the work of Thorold Rogers, by Professor W. J. Ashley of Toronto University; a demonstration of the "radical unfairness" of representation in Connecticut under the town-rule system, by Clarence Deming of New Haven; a discussion of farm mortgages, by an Illinois farmer, W. F. Mappin; a strong attack upon the policy of the general land office as regards the "indemnity lands" granted to the railroads, by Fred. Perry Powers of Washington, D.C.; a statistical paper upon Italian immigration, by Hon. Eugene Schuyler; the first of two papers upon the materials for English legal history, by Professor F. W. Maitland, Downing professor of law at Cambridge University, England; and the usual number of book reviews.

INDUSTRIAL NOTES.

Electrical Apparatus Abroad.

INFORMATION has reached us that the Sprague Electric Railway and Motor Company has recently closed quite a large contract for electric street railway apparatus with the principal street railway company of Florence, Italy, for the equipment of their line. This apparatus includes overhead system of the regular Sprague type, ten complete car equipments using two thirteen horse-power motors on each car, and station equipment complete.

This will be the first installation of American street railway apparatus abroad, where the progress in electric railway science has been very slow. The present method of running street cars in Florence is partly by animal power and partly by small steam dummies; and it is thought that the electric cars which combine the safety of the horse-car with the speed of the steam-car, and are much cheaper to operate than either, will have a large field to fill. It is said that this equipment is only a small portion of a very large equipment which will be ordered by this company, and if the result proves successful, it is thought that very many other Italian cities will adopt electricity for their street cars.

Electricity at Cleveland.

Cleveland can now be called properly the electric city of the West. In a short time there will be over a hundred electric street-cars running over the principal streets of Cleveland, besides a large number of stationary electric motors in use in a great many varied industries throughout the city.

The history of the East Cleveland Street Railway Company, which was the first in Cleveland to adopt electricity on its line, is an instance of the success and satisfaction which electric street

railway cars are giving in every city where they have been installed. The first equipment of this company was installed by the Sprague Company about nine months ago, and included overhead line, station equipment, and sixteen electric motor cars. The proposition to install this line met with a great deal of opposition in Cleveland.

The electric line was to cover some of the most important and principal business and residential streets in Cleveland, but the equipment was finally installed; and after it had been put in operation, the citizens of Cleveland discovered that the neat iron poles and overhead erection were hardly noticeable, while the rapid transit afforded by the street cars was something vastly superior to the former slow service given, when the cars were drawn by animal power.

There have altogether been five separate orders given by the East Cleveland Company for electric car apparatus. The second order was for four additional cars, the third for eight additional cars, the fourth for eighteen additional cars, and a recent order placed with the Sprague Company by its agent, Mr. C. W. Foote, for thirty additional motors, making seventy-six motor cars to be operated on this one line.

Besides this road, there are two others in Cleveland; the Broadway and Newburgh, and the Brooklyn Avenue roads also operated by electricity. There is nothing which speaks more highly for any kind of apparatus than the indorsement by its users, and there is no indorsement more convincing than the continued addition to an original equipment. The results, therefore, at Cleveland prove conclusively the good results and satisfaction given by electric apparatus when applied to street railways, and cannot be too commendatory of the style of motors used.